DISCUSSION ON

"STANDARDS AND MEASUREMENTS"

AT THE CONVENTION ON SCIENTIFIC RADIO, 7TH APRIL, 1948

Capt. C. F. Booth: With reference to the paper by Drs. Hartshorn and Essen, I should like to emphasize the importance of standards to the progress which has been achieved in radio-communication during the past 25 years and to present developments at ultra-high and super-high frequencies.

Considering one of the standards to which the authors refer, the standard of frequency, it is of interest to recall that 25 years ago international measurements of frequency agreed to about one part in 1 000, whereas to-day they agree to within two parts in 100 000 000. The National Physical Laboratory has been intimately concerned with this remarkable advance. It may be asked whether the radio engineer is really interested in such precise measurements. In fact, he is, and there are many radio transmissions for which the frequencies must remain stable to one part in 10 000 000 over many hours. If this performance is to be realized at a transmitting station, it is important that standards should be available which are one order better.

The standard of frequency is calibrated in terms of time by counting the number of vibrations which it makes in a specific time interval, and, generally speaking, the more precise the time interval, the more precise the determination of the mean frequency of the standard over the interval. There has, over the past few years, been some friendly rivalry between the astronomer, defining the time interval, and the radio engineer producing frequency standards. To-day the stability of the frequency standard, while perhaps not rivalling, is definitely approaching the stability which is achieved by our fundamental standard of time, the rotation of the earth.

The authors say that any further advance can be achieved only by a considerable improvement in both the frequency stability and the long-term reliability of quartz clocks. I suggest that it is opportune to consider whether the approach which has been made to the design of the quartz clock is the correct one. In developing standards of frequency, most workers have been concerned with producing a device capable of very good frequency stability both over very short periods (seconds and minutes) and very long periods (months and years). This has necessitated the use of very complex equipment. I suggest that the requirements of the quartz clock are not the same. A stability of one part in 10^9 over minutes is not required. What is required is a compact and relatively simple piece of equipment, capable of operation for ten years without a failure, with a closely predictable rate over the ten years, small frequency aberrations over minutes not being important. This approach to the problem of quartz-clock design leads to a very different solution from that given by the present design, which is a frequency standard in all but name.

The slide which the authors showed, giving the performance of three quartz clocks of different types, was very interesting, as it illustrated three different approaches to the problem in terms of quartz vibrators, giving the same answer with an agreement within 2 in 10^8. The authors did not consider in detail the causes of those variations; I agree with their view that the vibrations are due in part to surface phenomena on the quartz vibrator, to the mounting system and to the drive; the drift of the mean temperature of the associated temperature-controlled unit may also be a contributory factor.

The standard frequency is, and will continue to be, a very important factor to the radio engineer concerned with the provision of circuits, particularly in view of the increasingly tight tolerances now being set by international radio regulations.

Mr. C. W. Oatley: Referring to the paper by Dr. Jones, I should like to draw a distinction between bolometers and power-measuring instruments on the one hand, and voltmeters and ammeters on the other. There is one essential difference between these two types of instrument, in that bolometers make their measurements by absorbing the power available, and the power cannot be used simultaneously for any other purpose, whereas voltmeters and ammeters are generally inserted in the circuit in the hope that they are not influencing the circuit. I suggest that voltmeters and ammeters cease to be useful at frequencies where their effect on the circuit is so large that the voltage or current which they measure is not even approximately that which appears when the meters are removed. If we accept this distinction, we must consider at what frequency voltmeters and ammeters really cease to be useful instruments. It seems to me that 300 Mc/s is on the high side, and I suggest that above, say, 100 Mc/s we do not get much useful information from a voltmeter. Admittedly, if we are prepared to leave the voltmeter in the circuit all the time, it can be a useful monitor, but it is no longer a primary measuring instrument.

This question of the point at which to change over from voltmeters and ammeters to bolometers is of some practical importance, because it determines in what units signal generators should be calibrated. At low frequencies we calibrate signal generators in volts and microvolts, and at very high frequencies the calibration is in watts and microwatts. The change-over must be made at some point, and there is something to be said for having fairly general agreement as to the frequency of the change-over point. I think that if some agreement could be reached it would be of value to instrument manufacturers. It may very well be that there is some range where both types of calibration are useful, but I do not feel at all certain about that, and I suggest that 100 Mc/s is perhaps the point at which we should change over from volts to watts.

I think that this 100 Mc/s point really marks the change-over in methods of measuring impedance. At frequencies below 100 Mc/s, bridges form on the whole the best way of measuring impedance, because standing-wave methods below 100 Mc/s lead to very bulky apparatus. Above 100 Mc/s, standing-wave

methods and resonance methods are probably more accurate than bridges. I do not suggest that bridges cannot be used above 100 Mc/s, but I think that the other methods are probably more accurate.

Referring to Mr. Colebrook's paper, I was interested to learn that the estimate of accuracy of power measurement which I made two years ago is still not far from the truth; that is to say, for the measurement of high powers about 1% by calorimetric methods, and for low powers by bolometer methods probably 1 db at 10 cm and possibly 3 db at 3 cm. We can make measurements of powers of the order of 1–10 watts with an accuracy of the order of 1%, but when we come to milliwatts we incur errors varying between 10% and 50%. During the war and immediately afterwards that did not matter very much, because the low-power measurements were used chiefly to decide whether one receiver was or was not slightly better than another, and as long as everyone used the same type of bolometer the absolute accuracy did not matter very much. The bolometer measurement, however, is the basis of field-strength measurements; it is used in measurements of propagation of radio waves, and the measurement and theoretical studies of noise. It is important, therefore, from many points of view that we should be able to make low-power measurements down to the lowest wavelengths with the highest precision possible.

I suggest that there is a strong case for carrying out an experiment in which power is measured at a level of the order of, say, 1–10 watts, is attenuated down to a level of 1 mW, and the resultant 1 mW, which will be known, compared with 1 mW as measured by a bolometer. I believe that this comparison might improve the absolute accuracy of the measurement by a factor of 10.

Since I made my estimate a good deal of useful work has been done on piston attenuators, and we know much more about them than we did two or three years ago. They can now be relied on to give quite high accuracy, and it is time that we cleared up this outstanding uncertainty in power measurements at low levels.

Mr. S. B. Smith: With regard to the paper by Dr. Smith-Rose, I think that both national and international standards of the accuracy and methods of field-intensity measurement are long overdue. The work carried out by the National Physical Laboratory and a few industrial research laboratories needs considerable expansion if the apparent lack of adequate standards is to be remedied.

One of the reasons why industrial agencies do not put much effort into such studies is that considerable man-power (with small productive output) is involved in carrying out, not only instrumental development, but fundamental research. It seems that the main avenues for producing standards will rest to an ever greater degree upon the National Physical Laboratory, and possibly upon operating agencies.

The author raises the question of accuracy and mentions the U.R.S.I. recommendations of 1946 as a target for further study. I find the proposed accuracy of 0·4 db for fields of 1 μV/m (in bands 1–3) rather better than is likely to be achieved. I think these suggestions are based on the accuracy of current and voltage measurements. Factors such as small aerial pick-up, first-circuit and Schrott noise, atmospherics and interference, fading and other propagation phenomena, combine to make these low-level accuracies desirable, but unrealistic, at least to the communication engineer.

If the overall accuracy of these weak-field-intensity measurements were about 2 db, the target figures suggested by U.R.S.I. might be nearer practical realization in our time. The high-level accuracies associated with bands 1–3 also seem to have small margins when the present-day accuracy limits of current and voltage measurement are borne in mind. I should like to see the accuracy of field intensities of 1 V/m compared, using various national standards. The large discrepancies mentioned by the author, even in the 100–500-m band, have to my knowledge not been reduced to any marked degree as a result of recent work. At the time these tests were carried out (1936), was it at all certain that the N.P.L. standards were superior in accuracy to commercial equipment?

With regard to bands 4–5, I question the need or wisdom of suggesting such a precise low-field-intensity performance, particularly in view of the many factors entering into the overall measurement. A well-designed loop-aerial system in association with an extremely-narrow-bandwidth receiver will be needed if such high precision at 0·1 μV/m is to be instrumentally attainable (assuming that interference and atmospheric noise are not the limiting factors). Here again I suggest that 1 μV/m with a target accuracy of 3 db would be more practicable that the value suggested by U.R.S.I., even if it is not the communication engineer's ideal.

In all such low-level measurements the bandwidth of the receiver requires specification, particularly when the various U.R.S.I. bands are under consideration, certainly when fields of the order of 0·1–1 μV/m are the minimum intensities to be measured. Perhaps U.R.S.I. will study this very important aspect in relation to their field-intensity recommendations.

From a scientific point of view, it is clear that a complete knowledge of an incident field cannot be obtained unless the magnitude and phase of three vectors are simultaneously measured. Present-day aerial techniques (particularly those associated with ionospheric-ray propagation) are crude and at best only represent a simple and by no means error-free measuring method. I feel that many of the loosely-quoted accuracies (whether on low- or high-field intensities) can be accepted only with caution. Much research will be needed before any national standards are within sight of meeting the U.R.S.I. study targets.

At the present time it seems clear that only simple ray paths with known polarizations and great-circle tracks can be measured to within a few decibels. For many applications most complex instrumentation coupled with statistical analysis will be necessary before the complete assessment of many communication paths can be realized.

The scientific research problems associated with the field-intensity measurement of atmospheric disturbances (bands 1–4) seem to be more complex than those considered by the author. It is hoped that considerable expansion of these studies (particularly in the U.R.S.I. bands 1–4) will be made by the Department of Scientific and Industrial Research. Communication engineers need such data in order to verify and supplement the Central Radio Propagation Laboratory noise-grade charts. These charts have been based upon scanty data coupled with much speculative analytical guessing; they are most useful, but considerable and extensive international co-operation is still needed if improved charts are to be produced.

The question of expressing field intensities (at wavelengths shorter than, say, 1 metre) in microwatts per unit area seems to require international agreement. To an older generation the idea is not novel, but at low frequencies I find the microvolt-per-metre specification adequate for all normal communication engineering. At centimetre wavelengths, on the other hand, the power-per-unit-area concept has many attractions. I should like to know the author's views on the merits and drawbacks of both methods of expressing the radiation intensity.

I suggest that field-intensity studies relating to wanted radiations and noise are just as important to engineers as basic ionospheric data. It is to be hoped that the Department of

Scientific and Industrial Research will expand its efforts in these fields of study and international co-operation.

Mr. W. H. Ward: Dr. Hartshorn spoke of the possible direct connection between line-spectra and frequency standards. I should like the delegates to the forthcoming international conference to consider a wide extension of the ideas lying behind this. The conference may establish a third landmark in decisions on electrical standards. The first landmark was the decision, made about a hundred years ago, to use as standards direct currents rather than the fields associated with them. The fields are important in calculations and theoretical work, but the experimenter could be tied down to what seemed at the time to be an arbitrary thing, a current. This has been very successful. About fifty years ago, someone whose name seems to have been forgotten invented the concept of impedance. It was a simple extension of the resistance that was used with direct currents, but it was by no means obvious at the time. Without it, the development of a.c. measurement would have been very difficult and complex.

It seems possible that a new landmark is now needed. Just as alternating current grew on to direct current, so now we have centimetre waves growing on to radio that is part of alternating current. Now is the time to decide the standards that are most natural to the experimenter. I cannot produce the answer ready-made, but pointers can be found in the experience of the last few years. The practical worker on centimetre operation does not use current or voltage, but he uses fields. His fields are often enclosed within conductors, and they are the form in which electricity shows itself to him. They are what he measures. I suggest that, before the conference, it would be profitable to consider whether the best standards for centimetre waves are standards that apply direct to contained fields. It is fairly certain that, with centimetre waves, voltage and current are not used. What has not been referred to, however, is that the experimenter does not use impedance either. The theoretical worker does, but the experimenter does not; he works in a self-contained system in which fields control almost everything. The form of the field is to him a sort of translation of what impedance is to the worker at lower frequencies. It may be possible to use this state of affairs to decide on the standards which will be the most useful for many years to come.

I think it is very appropriate that our delegates should seek to be briefed on this subject, for it is traditional for the engineers and scientists of this country to take longer views than other people, and this is a case where a long view can be taken and will pay handsomely.

Mr. D. Maurice: Dr. Hartshorn and Dr. Essen say that inductance and capacitance standards can now be measured at radio frequencies to an accuracy of about 0·01%. I think that statement applies to only very few standards and gives too optimistic a view of what can really be done. The authors refer to an inductance of 0·1 μH. Some years ago I sent to the National Physical Laboratory a coil which I thought had a value of the order of 0·1 μH and asked for it to be measured at 1 Mc/s. It was sent back to me three years later and had been measured in a range between 4 and 6 Mc/s; the device which I wanted to check with that coil could not measure in the range of 4–6 Mc/s, so that the N.P.L. measurement was of no value to me.

I believe that the N.P.L. has a standard of inductance which consists of two very big coils coupled together, but unfortunately it is not possible to use it as a standard for smaller inductances. Although we may possess a standard for a henry or a millihenry, therefore, this is useless for checking inductances of a microhenry, or possibly 0·01 μH. It seems to me that a whole range of standards of inductance is necessary, from the largest values that are used, down to, in my case, 0·001 μH.

Dr. Jones refers to attenuators. I feel that it might be possible to make a useful attenuator at a fixed medium frequency, not greater than 20 Mc/s and not less than 1 Mc/s, with wide-band radio-frequency transformers. The only radio component which can easily be made in a laboratory is a transformer, because the number of turns can be counted. This is so particularly if it is a radio-frequency transformer having only a few tens of turns. The only thing that falsifies the transformation ratio is the leakage inductance, and that can be checked in a variety of ways. It can be measured, and it sometimes comes to less than 0·1 μH. If the transformer has a coupling factor k, the open-circuit voltage produced by that transformer is nk, so that, if k is known to equal, say, 0·996, then the turns ratio is multiplied by k, so that for a known input voltage, the output voltage is known to within about 0·04%. I have not tried this scheme yet, but I believe that it offers hope of producing an accurate intermediate-frequency attenuator.

Dr. Oatley apparently wants a decision made as to the frequency at which we should cease measuring volts and amperes and start measuring watts and microwatts. I do not think we should attempt to make hard-and-fast rules, and to fix the frequency above which we should use standing-wave meters and below which we should use bridges. As Mr. Ward has said, the impedance concept might not be very useful, but certainly in the v.h.f. band there is a very strong case for the use of a bridge, if only someone could make one which worked properly. Bridges now work up to about 10 Mc/s, as Dr. Jones says, and I feel he has been a little cautious there, because they might be said to work up to 50 Mc/s when used correctly.

Mr. J. H. Briggs: I should like to comment primarily on the papers by Dr. Jones and Mr. Colebrook, principally because I feel that they have left out what I should have thought was an important point for any international conference. Dr. Smith-Rose brought out the consideration of target accuracies for various types of measurement, and I think that that is a factor of some importance in considering where it is necessary to work. There has been insufficient emphasis in the other papers, I think, on the sort of laboratory measurements which are required and on how far the existing types of measurement are successful in meeting the needs of the experimenter.

As far as I am concerned, I do not think that there is any doubt that the most serious situation arises in regard to power measurements at high frequencies. As Dr. Oatley has pointed out, there is good reason for believing that checks between high-power and medium-power measurements can go a long way towards improving the accuracy of the medium-power measurements, but I think it is safe to say that it is essential for any real progress in radio work in the centimetre field that we should be able to measure power in the medium-power field to at least one order of accuracy better than is possible at the moment.

I think that the present situation is unsatisfactory also in respect to pulse measurements. One of the principal changes, it seems to me, in radio measurements during and since the war is the greater use which is being made of pulse technique. Interference measurements by means of pulses is one example. With servo systems and amplifiers there is a tendency nowadays, instead of checking stability by measurement of amplitude and phase, to check that stability by the performance on transients. Also, of course, radar and television require considerable work on pulses.

I consider that the situation is far from satisfactory in the pulse field, primarily because of the absence of any real definitions of pulses and of quantities which can be measured practically in a simple fashion and which can be used by the radio engineer

in his design work. Suggestions have been put forward from time to time, but I think that we must as soon as possible have available some laboratory standards against which we can make pulse measurements. For those standards to exist, we must have some agreed definitions, and I should like to see some action taken towards getting such definitions.

Mr. R. Street: In connection with Dr. Oatley's remarks on the extrinsic calibration of high-frequency milliwattmeters, I should like to report that recently I have been able to perform this experiment using a method which is essentially the same as the one proposed by him. A length of Pyrotenax cable, the attenuation of which (about 42 db) was measured by a piston attenuator, was incorporated in a constant-flow calorimeter. It was estimated that the power input to the cable could be determined from temperature-rise measurements, etc., to an accuracy of about $\pm$ 1%, but the overall accuracy of the calibration (3–4%) was limited by the accuracy of determination of the attenuation of the cable. However, one of the Ministry of Supply establishments is to co-operate in this work, and it is hoped to reach a higher order of overall accuracy in the future.

Up to the moment only S-band milliwattmeters have been examined, and it may be of interest to mention at this stage the results which have been obtained with a thermistor milliwattmeter designed by Mr. R. C. Robbins of R.R.D.E. When the input power to this instrument was 0·97 mW (determined by calorimetric measurements, etc.) the power indicated was 1·00 mW; thus, within the rather wide limits of experimental error, this instrument reads true power.

Since the method is of general application, it is hoped to extend this work to frequency ranges other than the S-band.

Dr. J. S. McPetrie: Dr. Smith-Rose has mentioned that there is a tendency to calibrate field-strength measuring sets on frequencies above about 30 Mc/s by observing their performance on locally produced fields of known strength. In my opinion, this radiation method is preferable for the following reasons to that used on lower frequencies, where a signal generator built into the field-strength measuring set is used to inject a known voltage into the receiver: The measuring set is calibrated direct in terms of field-strength and under the conditions in which it will subsequently be used; the relatively complicated calibrating equipment is divorced from the field-strength measuring set, thereby reducing the weight of the latter; and the same calibrating equipment can be used for measurements on a number of sets.

I suggest that an effort should be made to apply the radiation method to calibrations at longer wavelengths. Dr. Pressey and I used it in a somewhat primitive way some 12 years ago at wavelengths of the order of 1 000 m with satisfactory results. I feel that the method shows good promise of providing a convenient and accurate method for the calibration of field-strength measuring sets at all wavelengths.

Mr. H. L. Kirke: I should like to mention a somewhat abstract point in connection with the measurement of impedance. What is an impedance when we measure it, and how do we measure it? Supposing we had a balanced open-wire transmission line with wires 10 in apart and (to exaggerate) used that at 3 metres. How do we measure its impedance, and, when we have measured it, what do we mean by the impedance? We are talking about a pair of terminals 10 in apart. If we measure the impedance on a bridge, the terminals of the bridge are probably less than 0·5 in apart. We can only measure in terms of the leads connecting the bridge to the impedance.

On the other hand, if we want to make components in a laboratory, we have to consider their physical size and layout, and, in fact, what is meant by the impedance. If we take a coil, having, say, six turns, which is 2 in long and measure it on a bridge the terminals of which are 0·25 in apart, there are leads from the bridge to the terminal of the coil. What, then, do we measure?

If we measure the impedance of an ultra-short-wave aerial at the top of a mast, which particular part of the aerial do we measure, and, when we have measured it, what does the result mean? Basically, we want to know the field impedance, whatever that may be.

Dr. Booker presented a paper* on the impedance concept in 1946. I do not offer to provide a solution, but to point out certain difficulties which have been discussed at great length in my own laboratories, where we have been concerned with the measurement of impedance, particularly using bridges. Measurement by means of a bridge is very satisfactory up to about 30 Mc/s, and with expert use it is satisfactory up to about 45 Mc/s; it is not yet satisfactory but is under development up to 100 Mc/s. Bridge measurement both in the field and in the laboratory is much quicker and much more convenient than other methods, and is therefore well worth while developing where any extensive measurement programme is envisaged.

Coming to the next step, from impedance to field-strength measurements, many engineers will be almost shocked by the statement that in a large number of cases it is possible to measure field strength to an accuracy of only about $\pm$ 20%, which corresponds to $\pm$ 44% in power. One of the ways of measuring the efficiency of a transmitting system is by measuring the field strength, and power costs a great deal of money at the 100-kW level. In the U.S.A., the specification for a broadcasting station includes the measurement of field strength at a distance of 1 mile. I do not think that they specify, however, exactly what the accuracy of the field-strength measurement should be. One of the points which has recently arisen is that it is not the slightest use for an engineer to write an exact specification unless he specifies how the measurement is going to be made. We must have standards in all these matters, and standards or tolerances of standards must be established only when they can actually be measured.

Dr. J. L. Pawsey: I should like to draw attention to the remarks made by Dr. Hartshorn, concerning the possibility of producing a frequency standard by means of an absorption line in some suitable form of matter. I think that this represents the development which has the greatest potentiality for the improvement of radio technique which has been discussed so far before this Convention. In New York, Dr. Hershberger† recently described an actual stabilized oscillator working on a wavelength of about 1 cm based on an absorption line in ammonia and having a stability of about 1 part in 10^6. That, of course, is crude compared with the ultimate stability of the best quartz crystals, but it is a first attempt.

In the measurement of time it offers the possibility of the sequence of developments which we have seen in the case of the definitive standard of length. In that case, the original metre was an astronomical unit, connected with the size of the earth. It could not be reproduced so well as the distance between two marks on a piece of metal, and the definitive standard was changed to the present metre. We are now thinking of a change to a standard based on the wavelength of light. The corresponding sequence in time measurement is in the first stage. Our present standard is an astronomical unit. We now have quartz-crystal oscillators which have a greater accuracy, so that there is a possibility of making a change to such a standard. This would correspond to the metre, but may not be satisfactory because of lack of durability. If the next step could be made, i.e. the change to a standard dependent on a property of a particular form of matter, the advantage would be obvious.

* "The Elements of Wave Propagation using the Impedance Concept," *Journal I.E.E.*, 1947, 94, Part III, p. 171.

† HERSHBERGER, W. D., and NORTON, L. E.: *R.C.A. Review*, 1948, 9, p. 38.

Mr. C. H. J. Beaven: I should like to point out that the standards of length and time mentioned in the paper by Dr. Hartshorn and Dr. Essen are related in the measurement of a velocity, the velocity of light. Is it possible, therefore, that velocity is more fundamental than length or time, and that a system could be formulated with velocity as a basic unit?

Mr. D. H. Hughes: Mr. Oatley questions whether 300 Mc/s is the best frequency at which to change the technique employed for certain types of measurement from that used at lower frequencies to that which is practised at centimetre wavelengths: he suggests that 100 Mc/s would be a more suitable boundary. I regard the latter as an unsatisfactory choice, since it would divide a part of the v.h.f. band which is becoming very widely used for fixed and mobile communications. In my opinion, the present boundaries between the h.f. and v.h.f. bands, i.e. 30 Mc/s, and between the v.h.f. and u.h.f. bands, i.e. 300 Mc/s, remain the most suitable for any changes in method which become necessary as the frequency is increased. Furthermore, I think that h.f. measurement techniques should be retained as far as possible for the v.h.f. band, since the majority of engineers working in the v.h.f. band have been trained in methods used at lower frequencies rather than at higher frequencies. The construction of v.h.f. equipment has evolved mainly from that of lower-frequency equipment.

Certain h.f. practices, such as the measurement of r.f. current by means of thermal ammeters, are not suitable for v.h.f. working, but I see no reason why the use of signal generators calibrated in microvolts should not be retained up to 300 Mc/s, and there appears to be no advantage in rating the performance of receiver r.f. amplifiers in terms of power gain rather than voltage gain. The fact that v.h.f. signal generators at present available are unsatisfactory in certain respects is no reason for abandoning their further development within the framework of normal test procedure: it is not so long since m.f. signal generators were equally inaccurate, and, no doubt, the manufacturers of test equipment will in due course be able to overcome their difficulties in the v.h.f. band.

Dr. Jones doubts whether the microvolt can be realized to better than $\pm 0{\cdot}5$ db; presumably he is referring to the accuracy of instruments used as standards. So far as commercially available instruments are concerned, the accuracy is far from approaching this figure, even at low frequencies. At very high frequencies, particularly at low levels, errors ten times as great are common even in so-called "standard" signal generators.

Dr. J. A. Saxton: I should like to follow up the question of the frequency at which a change in measurement technique must be made, and, in particular, I should like to consider the frequency up to which it is desirable to be able to make direct measurements of r.f. current. It seems to me that in any case this change-over frequency depends very much on the type of measurement concerned. From the point of view of the calibration of field-strength measuring equipment, I prefer to make determinations of current at least up to, say, 500 Mc/s. This procedure would, in fact, appear to be possible, since Dr. Gainsborough showed some time ago that carefully-made thermocouple milliammeters could be relied on for the measurement of r.f. currents up to 700 Mc/s when used in a suitable manner. This is a matter of some convenience in the setting up of a known radiation field for the calibration of a field-strength measuring set, since I think it is desirable to retain the simple half-wavelength dipole as a standard radiator up to as high a frequency as possible.

Dr. McPetrie has suggested that the establishment of a known field by the radiation method might be attempted at frequencies much less than the lowest at which it has so far been applied, which is about 15 Mc/s. Such an attempt is likely to meet with grave difficulties, but that is no reason for not making it. Dr. McPetrie and I showed that, with care, a good agreement could be obtained between the radiation and direct-injection methods of calibration of field-strength measuring equipment at 15 Mc/s, and I think that about 10 Mc/s is likely to prove the lowest frequency at which the radiation method may safely be applied. The radiation method becomes particularly powerful as the frequency is increased to the order of 100 Mc/s, and in my opinion the region of, say, 50 to 300 Mc/s is probably the one in which we can measure field-strengths most accurately.

On the question of accuracy, and in particular regarding the lower limits of field strength which were indicated by Dr. Smith-Rose, I think that Mr. S. B. Smith's comments were a little pessimistic. I do not see why we cannot measure field strengths as low as those indicated by Dr. Smith-Rose's slide. There is every reason to suppose that in some of the frequency bands the accuracies indicated on that slide, and the lower limits, can be achieved, particularly in the v.h.f. region. As regards the lower limits at even higher frequencies, the matter is largely governed by the aerial system which we use, and at centimetre wavelengths high-gain aerials can be used, thus achieving relatively high sensitivities.

Mr. H. M. Smith: I should like to refer to the standard of time mentioned by Dr. Hartshorn. That is the one standard which is the responsibility of the Royal Observatory rather than of the National Physical Laboratory, and it possesses the disadvantage that its determination requires constant work. The definitive standard (adopting Dr. Hartshorn's term) is, of course, the earth, and the rotation of the earth is subject to many irregularities. The definition of Greenwich mean time takes into account some of those irregularities, but there are others which are now becoming of definite importance, in particular those due to polar variation. Since 1946, provisional corrections for polar variation have been used in assessment of the performance of the Greenwich clocks. In addition to the international meetings being held in connection with U.R.S.I., this summer there is also to be a meeting of the International Astronomical Union, and it is hoped that some consideration will be given to a new definition of time on a more uniform system, in which allowance is made for some additional known variations. Such an improved system would, of course, be appreciated by those who wish to check frequency standards.

The greater precision in timekeeping, and in particular, the improved performance over long periods consequent on future developments in frequency standards, will enable the astronomer to find out more about the irregular fluctuations in the earth's rotation.

From observations of the orbital motions of the moon and planets, there is evidence of variations in the length of the day. It is not possible, however, to determine over how long a period these changes take place. The increasing precision of quartz clocks, combined with improved astronomical observation of time, may provide independent and more precise evidence of such changes. There is no reason to suppose, however, that it will ever be possible to predict these irregularities in the period of the rotation of the earth, and the definitive standard of time must therefore be regarded as rather imperfect.

With reference to Mr. Ward's remarks relating to ultra-high-frequency technique, it may well be that above a certain frequency a standard in terms of wavelength may be preferred to one involving the unit of time.

Dr. C. H. Westcott: I should like to support Mr. Oatley's remarks concerning the frequency at which we should draw the line between the two types of method of impedance measurement. During the war I was associated with a group which made impedance measurements in the doubtful region around 200 Mc/s. Our first problem arose just at 200 Mc/s, and it was solved by

adopting standing-wave methods. I feel very little doubt that the correct method for impedance measurement at that and higher frequencies is the standing-wave method. We used this method at frequencies as low as 75 Mc/s, because suitable equipment was available. I am not sure that the dividing line should be placed as low as this, but I do not think 100 or 150 Mc/s would be far out. I do not know of any equipment developed since that date which would change that opinion.

For measuring voltages and currents, I think the question is a little more open, and the line might be drawn anywhere between 100 and 600 Mc/s. We made relative voltage measurements at frequencies up to 600 Mc/s, but to do so we used components (e.g. vacuum thermocouples) which actually measured power. I should like to advocate the use of the concept of power in preference to voltage for many problems at these frequencies. I think that we should often use power flow in watts/m^2 rather than field strength in volts/m, particularly in aerial and propagation work. A difficulty is that one often obtains rather small numerical values, and it would be convenient to have a name for the micromicrowatt/m^2.

Mr. Kirke asks what is an impedance. It seems to me that this is related to the mode of propagation which we are considering. We used impedance almost in the sense of a derived standard on transmission lines; we had a line of given geometry and measured the standing wave, and that to us defined the impedance. So long as one considers only one mode, there seems to be a definite meaning for the term impedance, but if one has a waveguide, operated at such a frequency that more than one mode is possible, the impedance is different according to which mode is excited.

We also worked with screened twin lines, with which there are always two modes, a balanced (normally the wanted mode) and an unbalanced mode, and these two have different impedances. The case that Mr. Kirke mentioned of an open line without a screen may have one or more unbalanced modes as well as the wanted mode, but, unless we say which mode we are considering or which mode we are exciting, we cannot define the impedance. If, however, we do define specifically the balanced mode on an open-wire line, we can define exactly the impedance at any point along the line.

Mr. Caradoc Williams (*communicated*): It is my opinion that the subject of receiver bandwidth and its relation to instrumental accuracy of field-strength measuring equipment has not been given sufficient prominence by Dr. Smith-Rose. Doubtless, it will be generally acknowledged that the accuracy of field-strength measurement is greatly facilitated when the signal/noise ratio is high. When the signal and noise approach comparable amplitudes, the accuracy of measurement is usually of a low order due to the law of the detector. Under these conditions, I have found it advantageous to calibrate the field-strength receiver for different levels of injected noise. The noise is measured in the absence of the signal, and the signal is then measured with the noise. From these two measurements the signal strength is determined.

Some radio receivers in use on navigational aids have a bandwidth of about 100 c/s and provide reliable operation on a signal/noise ratio down to unity. Receiver noise is approximately proportional to the square root of bandwidth. Thus, the signal/noise ratio can be improved considerably by the reduction of receiver bandwidth, for example by reducing the field-strength receiver bandwidth from 5 kc/s to 100 c/s, the signal/noise ratio can be improved about 7 times, resulting in very considerable improvement in accuracy of measurement of small signals. I suggest that field-strength measuring equipment should provide facilities for exploring the state of polarization of the received signal. In this respect, the conventional loop-receiving aerial is not very suitable.

THE AUTHORS' REPLIES TO THE ABOVE DISCUSSION

Dr. L. Hartshorn and **Dr. L. Essen** (*in reply*): Several speakers have commented on different aspects of the two regions which can be discerned in radio measurements. We suggest that the fundamental difference is that in the one region the *circuit* is the basic concept, while in the other it is the *wave*. The two fields, H and $\mathscr{E}$, are the common feature. They are not directly measured in either region, but are derived from measurements of current, voltage and power. Current is a concept of great scope in the circuit region, because a single value is applicable to a whole chain of elements. In the wave region its significance is more restricted, but not entirely lost, as Dr. Saxton has pointed out. Voltage also remains significant in the wave region, because it is voltage to which crystal detectors respond. Their indications are, however, of value only if the distribution of voltage is also known. The waveguide or transmission line, together with the conditions of excitation, define this distribution within limits, and therefore Mr. Ward's practical worker can reasonably claim to observe fields, for his "field" is essentially a voltage distribution of some reproducible pattern. His standing-wave ratio is determined as a voltage ratio.

Impedance, being defined as the ratio of voltage to current, is of great scope in the circuit region, but, as Mr. Kirke mentioned, its significance is modified as the wave region is entered, for, although the voltages between any two points is always definite, they are generally not characterized by the same current unless they are adjacent. The impedance concept is, however, so useful that we generalize it according to a convention which, while preserving its significance in the circuit region, makes it also applicable to waves by referring it to H and $\mathscr{E}$ fields as well as to the associated currents and voltages. The convention is such that transmission lines and waveguides of suitable construction become derived standards of impedance for the wave region, and it becomes natural to interpret standing-wave ratios in terms of impedance ratios.

Capt. Booth's suggestion that momentary perturbations might be tolerated in a long-period quartz clock would remain valid only if the perturbations were strictly random. Laboratory experience suggests that if any aberrations greater than those corresponding to Brownian movements are tolerated, the chances that they will prove to be strictly random over a long period are vanishingly small. As we have not yet reached the Brownian level, we still think that the line of attack we indicated is the correct one.

We fully agree with Dr. Pawsey about the potential importance of the absorption line as a frequency standard, but, when considering the analogy with the standard of length, it is necessary to note that the molecular absorption lines so far explored in the microwave region are less sharp than the atomic lines in the optical region that are used for the standard of length.

It goes without saying that the accuracy we quoted for measurements of inductance and capacitance could not be achieved for either very small or very large values of either quantity: the reactance must lie within reasonable limits, and moreover the residual inductance of the capacitor and the self-capacitance of the inductor must be reasonably small. To violate these conditions, as Mr. Maurice did, is to enter a region where general statements about accuracy become worthless. The real remedy is to keep standards out of it.

Dr. R. L. Smith-Rose (*in reply*): Mr. S. B. Smith emphasizes

the need for further investigation and development of the technique of field-strength measurement to meet the needs of the communication engineer. The translation of field strength into radiated power in the example quoted by Mr. Kirke also demonstrates that there is considerable room for improvement in the limited accuracies which are apparently attainable at the present time. The need for measuring the very low field strengths, referred to by Mr. S. B. Smith, was originally proposed by the practical radio engineers, who pointed out that many long-distance communication services on high frequencies are conducted with received field strengths of a few tenths of one microvolt per metre. The difficulties of the direct measurement of such weak fields are appreciated.

Although Dr. McPetrie and Dr. Saxton appear to have differing views as to the difficulties of establishing known radiation fields at low frequencies, it does not appear to be useful to pursue this discussion. The radiation method of calibrating field-strength measuring equipment was really introduced for use at the very high frequencies at which the calibration by direct injection of an electromotive force from a local oscillator was accompanied by difficulties and inaccuracies. Some speakers have drawn attention to the fact that at still higher frequencies it may be preferable to work in terms of power flux, rather than in terms of field strength. Such a change in technique will involve a determination of the direction of the radiation being measured, and careful consideration must be given to the relative advantages of the two terms before either national or international agreements on their use are made.